

Interactive comment on “A fully coupled 3-D ice-sheet – sea-level model: algorithm and applications” by B. de Boer et al.

Anonymous Referee #2

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In recent literature, a new and powerful method of simulating ice cover and sea-level variations through time has been developed in which the full interaction between ice dynamics and global, geographically variable sea-level variations is captured by coupling a dynamic ice sheet model to a gravitationally self-consistent sea-level model that incorporates viscoelastic deformation of the Earth. This method has been applied to simulate Antarctic ice cover variations and global sea level over the Last Deglaciation (Gomez et al., 2013). De Boer et al. have extended coupled ice sheet - sea level modeling to include multiple ice sheets in order to simulate global ice cover changes, and a traveling time window technique to facilitate longer timescale simulations over multiple glacial cycles. This work represents a substantial and novel contribution to the Ice Age modeling community, and is very appropriate for publication in Geoscientific Model Development.

My major concern with the manuscript is that the methods, assumptions and review of the literature are not clearly presented in the manuscript. There are many grammatical errors and typos throughout the text, and the description of the methods was hard to follow in places. I highlight these issues in more detail in my major comments and minor comments and corrections below. (Note: I noticed that the other reviewer attached a copy of the manuscript with some typos and grammatical errors highlighted. I worked from this copy so as to mostly avoid providing duplicate typo corrections.) The important modeling advances that de Boer et al. make are complex and subtle, and more care should be taken to explain them clearly.

As I discuss in my ‘major comments’ below, I request a sensitivity analysis to justify the choice of one key parameter in their model, and some justification of other modeling choices. Other than these suggested additions, the science and modeling aspects of de Boer et al., if I have understood them correctly, are valid and require no substantial revisions. The authors have provided useful supplemental information and some preliminary results from their model that look promising and support their conclusions. I would be interested to see further discussion of future applications and implications of their contributions in the concluding section.

In summary, their work makes an important contribution, and I would recommend publication of this manuscript following revisions, which may involve substantial changes to the text but not to the modeling or scientific conclusions.

MAJOR COMMENTS:

1. The text of the manuscript is unclear in places, contains many grammatical errors and typos and some jumps in logic that require further exposition. Further referencing is needed in some places as well (see minor comments and other reviewer’s typo corrections). Particular care should be taken in the Introduction section with the use of words like ‘since’, ‘however’ ‘but’, ‘eventually’, ‘clearly’, ... etc..
2. It would be useful to clearly define terms like GIA (does this term include elastic

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uplift and sea surface deformation due to ongoing/current ice distribution changes?), relative sea level (sea surface relative to solid surface, versus sea level at some time in the past relative to the PD), topography, and eustatic sea level, and then use these terms consistently according to your adopted definitions throughout the manuscript.

3. It was tricky to keep track of your terminology for time-related variables. I would suggest defining different names for (i) the time interval at which the models speak to each other, (ii) the set of times in the past at which the sea-level model retains information (I think you use Δt s for this, but sometimes the usage is confusing) and (iii) the time stepping in the ice-sheet model. Also, I found it confusing that in most of the text, $t=0$ refers to the start of the simulation (I think?) and the “future” is relative to the start of the simulation, but in plots, you refer to the start of the simulation at -410ky, and $t=0$ refers to the modern. I would recommend avoiding using the word “future” to mean anything but the future relative to present day.

4. I have a few questions about your eustatic calculations. In the uncoupled model, you calculate eustatic sea-level changes from changes in ice volume relative to present-day topography. There were extensive marine sectors of ice in the past, and the area of the ocean basin in the past was different from the modern ocean area. Does your calculation consider these differences? Is the eustatic calculation you input into the uncoupled model the same as the eustatic sea-level change inputted into the coupled model? Does it come from the $d18O$ data, or is it predicted from the ice volume changes in the models? If it is the former, perhaps state this clearly in section 2.2 and refer back to it when you use “eustatic” later? It is unclear why a eustatic sea-level change is used in the coupled model (I understand that that is what is happening from your discussion on p. 3515, first paragraph) when you could use the geographically variable, global sea-level change calculated in SELEV. Further explanation of how this eustatic term is calculated and incorporated in combination with the local geographically variable sea level in each ice-sheet domain would be useful. Does your coupled model conserve ice-ocean mass? Could some of the differences (or, rather, similari-

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ties) in eustatic sea level between coupled and uncoupled models (Figure 9) be related to the different methods of calculating it in each model?

5. Further explanation of the difference in treatment of solid surface deformation between coupled and uncoupled simulations is needed, as well as an explanation and/or reference to accompany your adopted Earth model parameters in section 2.3. Is the (3 layer) model you describe in section 2.3 different from the (2 layer?) model you adopt in Figure 5?

6. I understand that the ice sheet and sea level components speak to each other at 1000 year time intervals in your model, but there is no explanation of the choice of this coupling time. You mention that the ICE5G reconstruction adopts a similar intervals of 500-1000 years, but this is not sufficient justification for choosing a 1000 year coupling interval in your fully coupled simulation in which you highlight the influence of sea-level changes on the ice flow. 1000 years seems too long to capture the full influence of gravitational effects and elastic uplift on marine ice dynamics. Furthermore assuming a 1000 year step function in ice cover changes in the sea-level model could produce significantly different relative sea-level predictions, in particular in the vicinity of the evolving ice sheet, relative to assuming more smoothly varying changes in ice cover (perhaps larger than the residuals plotted in Figure 7). Have you explored using a shorter coupling time? Gomez et al (2013), for example, use a 200 year coupling interval for simulations over 40,000 years. In comparison, much of the extension in time of this modeling effort (i.e. from a 40,000 year model run in a recent study to a 400,000 year model run in this study) is achieved through simply using a longer coupling time interval of 1000 years rather than 200 years. I think that the (clever!) traveling time window methodology you introduce should have to potential to do better, allowing for shorter coupling time steps at a relatively low computational cost. Is this true? Or is the frequency with which the ice-sheet model stops to pass and receive fields with the sea-level model also a limiting factor? I would be interested in further discussion of this point in the manuscript. I think a sensitivity analysis is necessary to justify the coupling

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interval you choose in the simulations you present, since it is significantly longer than the one chosen in recent literature.

7. Related to the previous point, including further explanation of how the ice-sheet model responds to bedrock topography changes (i.e. how is the grounding line treated?), and providing the ice-sheet model time stepping would be helpful. The sensitivity of results to the coupling time will depend on this. I understand that this material is covered in other literature (de Boer et al. 2013), but since this treatment is key to understanding how the interaction between the two systems is modeled, it would be useful to include it here as well.

8. How is initial topography determined in the model? From section 2.1 line 25, it sounds like you adopt PD topography as your initial topography – is this correct? More detailed explanation of the topography adopted at the start of the ice-sheet model spin up and at the start of the coupled model simulation is needed. Topography in past interglacials may have been significantly different from modern topography. Since topography in the past is unknown, typically, an outer-iteration is added to correct the starting topography such that the model converges on the (known) modern topography at the end of the simulation (e.g. see Kendall et al. 2005). Do you have a sense of the bias you are introducing by not including this outer-iteration? This issue should be raised in the text and the assumptions you are making should be clearly stated.

9. I understand that the focus of this manuscript is on modeling with some preliminary results, but I would still like to see a discussion of how realistic your model results are. E.g. How well does your model capture what is known about ice extent and sea level over the last glacial cycle? How does it compare to other ice sheet and sea level reconstructions? How well does it capture modern topography and ice extent at the end of the simulation?

10. The preliminary results you show very clearly demonstrate that your model will be a powerful tool in providing insight into paleo ice sheet and sea level variations. I

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suggest you include a discussion somewhere of how you plan to apply the model in future work and what key problems you could use it to address.

MINOR COMMENTS AND CORRECTIONS:

title: There are a lot of hyphens of different length in the title. Perhaps remove those between “ice sheet” and “sea level”?

INTRODUCTION

P. 3507 4 - 'definitely' is colloquial and unnecessary here

5-6 - 'ice sheets' is repeated twice. Perhaps change to something like '... when the Antarctic and Greenland ice sheets extended...'

8 - 'In fact' is unnecessary

p. 3508 10- why 'However'?

14-16 - Further referencing is needed here.

27 - Grammar (As a consequence of what?)

26 to end of paragraph - referencing here is sparse, and missing entirely for rotational effects.

P. 3509 5 - 'According to GIA...' is grammatically incorrect. Do you mean 'according to the theory of GIA'? In addition, further clarification of what you mean by "GIA" could be helpful here. GIA is often associated with ongoing deformation associated with past ice and ocean loading effects, but in this sentence, I think you are referring additionally to the response in sea level to ongoing ice and ocean loading. Some clarification could be helpful.

3-11 - This paragraph is hard to follow. Land-based sea level records reflect both GIA effects and sea level changes due to ongoing ice sheet variations. In addition, on line 9, the wording 'since' implies causation between the first and second half of the last

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sentence, where I don't think there is any. Finally, on line 10, the wording 'eventually an RSL indicator' is confusing. Also, I think RSL here refers to being relative to present day, whereas it has a different definition in other parts of the manuscript. I would recommend reworking this section.

20 - Further referencing needed. You say "the sea level equation has been widely employed..." but only reference Peltier (2004) and earlier only Spada and Stocchi (2007).

21 - Grammar (incorrect use of 'However', and 'but' later in the same sentence)

23 - 'ice sheets evolution' - remove the 's', add a hyphen.

24 - Use of the word 'eventually' implies that at some point in time RSL changes do not define variations in topography and bathymetry. Is this what you mean? If so - I think more explanation is required.

25 - Distinguish between how the impact on marine ice is different from the impact on the "ice-flow pattern" in general, or remove the sentence on 26-27 if there is no difference.

28-29 - 'Thus far' is not true (e.g. Gomez et al. 2013's work that you mention below). Something like 'up until recently' would be more appropriate.

p. 3510 18 - What is your algorithm an alternative to? How is it different from the one used in Gomez et al. (2013)? The concept of a fully coupled model, and the algorithm employed (e.g. shown in Figure 2) have already been presented in the literature. Be more specific about what this study adds (e.g. a way of performing calculations over a long time periods and multiple dynamic ice-sheet models.)

METHODS

P. 3511 see typos highlighted by other reviewer. In addition. . .

25 - "We adopted" - you have switched to the past tense here. Also, see my major

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comment about initial topography.

26 - Is the Greenland topography name and reference here correct?

p. 3512 8 - typo - remove 's' from models

15-17 - remove hyphen from 'sea-level'. Also, See 'major comment' above about eustatic sea level.

25 - typo - represents

26 - typo - missing comma before "a temperature. . ."

p. 3512-3513 Section 2.2 – Does this section relate at all to the eustatic sea level used in some of the simulations? If so, explicitly saying that here would be useful. If not, disregard this comment and simply address my "major comment" about eustatic sea level above.

p. 3514 5 - self-gravitating, with a hyphen?

10 - 'current' implies that you change these settings later - is this true? If not, perhaps "for the results shown in this study", or 'default' would be more appropriate wording.

16 - self-consistent, with a hyphen?

17-18 – I suggest you revise to something like "... change depends upon all surface mass displacements (both ice and melt water) which have occurred. . .). You could also consider using the term "loading" instead of displacements.

22 - "We solved the model. . ." this sentence is awkwardly worded - I believe "solve" is not commonly used with "model".

27 - typo - sheets-shelf

p. 3515 2 - 'sub system' is one word or hyphenated?

5 - see major comment about eustatic sea level change

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11 - remove the 'with' after SELEN

15- 16 - space and time, instead of just space? Also, there should be commas before each "which"

19 - more explanation/justification of the 1000 year coupling time choice here would be helpful – see my major comment above.

p. 3516 6 – Add reference Milne et al. (2002) Quat. Sci. Rev. 21, 361-376?

23 - "Clearly" is unnecessary here. Also, what "time step" are you referring to here? 1000 years, or the ice-sheet model time step? How long is the latter?

p. 3517 Section 3.2 general comment - This section was very difficult to follow. It describes the most novel aspect of the modeling, so it is important to present it clearly. I found it challenging to keep track of all the variables, and also when the text was referring to a time in the past, a time interval, and a time in the future. I would recommend finding a new way to explain the traveling time window. You may also consider placing this section after the next section that introduces Figures 4 and 6, so you can make better use of these figures in your explanation.

15 - load Love, instead of Load love. Also, Peltier (1974) might be a more appropriate reference here?

16-19 - This sentence is confusing - perhaps it is missing some words?

p. 3518 11 - commas in the wrong place - no comma after follow, add a comma after l(t).

RESULTS

P. 3519 7 - by "overlapping", do you mean adding together? Do you also add in a eustatic term? See "major comment" above. More explanation of how the results in all the ice model domains are added together is needed.

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p. 3520 2 - does NT refer to a set of time steps or a number of time steps?

8 - It is not obvious why simulations here run for 480 ky but in the abstract and most figures you use 410 ky. I think I finally figured out that it could be related to the spin up of the ice sheet model?

10-12 - this sentence is confusing.

15 - linearly interpolated, not interpolations. Also, do you mean similar to Figure 4b, or as show in Figure 4b?

equation 3 - what are all of the terms in this equation?? As is, this equation is not helpful.

26 - why is the simulation time now 410 ky again?

p. 3521 7 - missing some words in this sentence? And the next? Why are the changes higher than present day? More explanation is needed here.

8 - "reach up to" rather than "go up to"?

20 - comma after 'variations'. Also - is your coupling time of 1000 years really short enough to capture the full influence of sea-level variations on ice sheet dynamics?

p. 3522 5-6 - fix grammar

5-8 - Gomez et al. (2013) also use the same treatment of Earth deformation and make this point.

9 - in section 2.3, you say you use a 3-layer model. Which is it?

17 - Perhaps add "treatment" after sea level?

28 - replace "that can be" with "reaching"?

p. 3523 2 - results instead of result

4-6 - this sentence needs rewording.

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14-15 - this sentence is confusing. . . I think you mean to refer to changes in RSL and eustatic sea level, rather than absolute values when describing this concept.

p. 3524 Final paragraph - This paragraph could be removed. It largely repeats what you have already said in earlier paragraphs of the conclusions. I would recommend instead focusing on the novel aspects of your model, i.e., the addition of the traveling time window and incorporating multiple ice sheets into your coupled model.

FIGURES: In general, I think you could include more information in the figure captions.

Fig 2: Can you explain how the T_{surf} is related to T_{NH} referred to in the caption?

Figs 4 and 6: It would be useful to add more information into these figure captions. For example, say where this bedrock deformation plotted is occurring. Is this bedrock deformation at a specific location on the globe? I think Figure 6 could be included as a frame of Figure 4 - it would be useful to see all these frames on one page. Finally, on Figure 6, I recommend adding an explanation in the figure caption of why the bedrock deformations stored jump from starting at 0, -5, and -10 ky, and then -60 ky (whereas in Figure 4c, you plot the black curves every 1000 years).

Figure 7: Include an explanation of the normalization of the residuals in the figure caption (or refer to where you explain it in the text). I am not sure if I correctly understand what the residuals are normalized by.

Figure 8: add “predicted using the coupled model” to the caption. Also – where does the eustatic come from?

Figure 10: I appreciated the supplemental movie. Perhaps refer to it in this figure caption so more people see it?

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